are really bacteria has been satisfactorily discussed by the researches of Bernard Renault who has placed the subject of bacteriology of fossil vertebrate remains on a safe footing. Those seen in the present sections often group themselves in pairs recalling the modern *Diplococci*. I have never seen chains of these forms in vertebrate material.

The other question as to how such minute bits of protoplasm are capable of preservation over many millions of years is one of those unsolved puzzles of paleontology which we may place with that of the fossilization of the ganoid fish brains from Kansas.

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QUOTATIONS

SCIENTIFIC PAPERS

ALTHOUGH the scientific societies made a valiant effort to preserve continuity through the war, the session now closed is the first that has been nearly normal for several years. Most of the younger men were engaged on work that does not qualify for membership of learned societies, and the scientific investigations of the others, young or old, were often advisedly kept secret. Now that science has resumed its old range and almost its old output the precise utility of the weekly and fortnightly meetings of the societies, under discussion before the war, is again being considered. Clearly they have a social value, increased by the almost universal change from the evening to the late afternoon, and by the more abundant presence of ladies, as members or as guests. But what of their specific function as an aid to the advancement of knowledge? It is to be confessed that for the most part this seems slight. Distinguished investigators are not always clear expositors by word of mouth. In many cases the programme is so long that many items, and these often the more interesting, have to be "taken as read." The actual communications made are often such that it is to be doubted if more than one out of ten of the audience has the slightest idea what it is all about. Sir James Dewar, speaking at the closing meeting of the Royal Institution, possibly partly in jest, ventured the opinion that it was good for people to listen to the most recent results of science, even if they failed to understand them. This is an opinion in which we can not concur, holding, on the contrary, that if there is a state worse than ignorance it is that of the vain worshippers of scientific shibboleths. If the purpose of a meeting is to convey instruction, the exposition should be as simple and clear as that to which Sir James Dewar himself has accustomed his audiences at Albemarle-street.

But the original purpose of the meetings of the scientific societies was to discuss new results rather than to educate. In earlier days, when the range of knowledge was narrower, almost any man of science was capable of emitting a useful impromptu opinion on almost any branch of science. An approach to such a communion between lecturer and audience may still be possible in some of the smaller and more highly specialized societies. In other bodies a useful attempt is sometimes made to reach it, by grouping the papers for a meeting, or by setting a topic for discussion. But even such arrangements frequently fail of their object, because those with most right to be heard are least anxious to criticize or to approve what they have heard for the first time, whilst those who have least claim to serious attention are most ready to hazard opinions. It would be interesting, were some society to experiment with a method frequently suggested, but, so far as we know, not yet actually adopted. It is the custom for the communications made at a meeting to be printed and published subsequently, after due examination by a referee. It is worth noting that strict precautions are taken to prevent substantial alteration or correction of a manuscript, even if the discussion had shown that these would be an advantage. There is threfore no gain by the delay, and much detriment to the value and interest of the meeting. If, on the other hand a paper were published in full, and distributed in the usual way at a due interval before the meeting at

which the author was to present it, experts and those with varying degrees of knowledge could master the main points of the thesis. They would thus be prepared to join in, or to listen to, a debate which would certainly be a real contribution to the progress of knowledge.—The London *Times*.

SPECIAL ARTICLES

ON THE LAW OF SURFACE AREA IN ENERGY METABOLISM [1]

The generalization that heat production in animals is proportional to the surface of the animal body rather than the weight of the body was first hinted at by French writers before the middle of the last century. It was formulated rather definitely by Bergmann in 1848 and was first placed on a definite footing of fact almost simultaneously by Rubner in Germany and by Richet in France in 1885. This so-called law of surface area has been quite generally accepted and has contributed much to the understanding of metabolism which we now have.

Recently this law has been submitted to severe criticism by F. G. Benedict and his colleagues[2] and the conclusion has been reached that surface area is little or no better as a measure of metabolism than is body weight. The purpose of the present communication is to direct attention to some natural limitations of the law of surface area which seem to have been overlooked by these critics. Harris and Benedict have rendered a service to the science of metabolism and nutrition by calling attention to the fact that since surface is usually expressed as a quantity in which two thirds power of the weight enters as a factor it must of necessity be less variable than the weight. As a matter of fact the mathematical relationship does not stop here; for in many instances the constant employed in the formula, for example, of Meeh or of Lissauer, by which the two thirds power of the weight is multiplied, equalizes the proportions between surfaces and weights. A few illustrations will make this clear. Suppose, for example, we have two infants weighing 7 and 8 kilograms respectively. Expressing their weights in grams and their surfaces in sq. cm. by the Meeh and Lissauer formulæ, we have the proportions shown in the first line of the following table. The ratio of

TABLE I.

Relation of Body Weights and Surfaces to Each Other

Weight Gm.	Ratio	Meeh-Rubner $11.9\sqrt[3]{(w)^2}$		Lissauer $10.3\sqrt[3]{(w)^2}$	
		Surface sq. cm.	Ratio	Surface sq. cm.	Ratio
7,000		4,354		3,769	
8,000	0.88	4,760	0.91	4,120	0.91
20 kgm...		0.8768 sq. m.		0.7589 sq. m.	
21 kgm...	0.95	0.9058	0.97	0.7840	0.97
40 kgm...		1.3920 sq. m.		1.205	
41 kgm...	0.98	1.4150	0.98 +	1.225	0.98 +
4 kgm...		0.299		0.259	
40 kgm...	0.10	1.3920	0.210	1.205	0.21
3.5 kgm...		0.274		0.237	
70 kgm...	0.05	2.021	0.135	1.750	0.136

weights is .88 : 1 and of surfaces .91 : 1. Now it is obvious that if the metabolism of these two children is proportional to their weights it must of necessity also be nearly proportional to surface. With two youths weighing 40 and 41 kilos the surfaces bear to each other exactly the same ratio as the weights, whether the Meeh or Lissauer formula be employed. Both, therefore, will be equally good measures of metabolism for the two individuals. The "discovery" that surface is no better as a measure of metabolism, than weight *as between individuals of nearly the same weight* could, therefore, have been made with paper and pencil.

[1] Abridged from an address delivered before the Yorkville Medical Society, New York City, March 21, 1921.

[2] Harris, J. A., and Benedict, F. G., "A Biometric Study of Basal Metabolism in Man," Carnegie Inst. of Washington, Publ. No. 279, Washington, 1919; Benedict, F. G., and Talbot, F. B., "Metabolism and Growth from Birth to Puberty," Carnegie Inst. of Washington, Publ. No. 302, Washington, 1921.